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## DISCUSSION

### PROFESSOR DODGE'S RECENT DISCUSSION OF MENTAL WORK<sup>1</sup>

By F. M. URBAN

The wider field of dynamic psychology comprises the study of all the conditions of mental phenomena; but it is legitimate to define an investigation of narrower scope as the science of psychodynamics, which is the doctrine of energy transformations conditioning mental phenomena. This definition is slightly different from the one given by Lehmann, who defines psychodynamics as the exact doctrine of the quantitative influence of simultaneous or successive mental states on one another. Mental work can not be defined in mechanical terms, nor by opposing it to play. Neither is it possible to give this term a consistent introspective connotation since introspective differences of mental processes are no indication of conditioning energy transfers. The common measure of mental work is number and time as is seen from the adding test, where the number of operations performed in a minute is used as a measure. This measure is useful for practical purposes, but it is essentially non-dynamic; and it is even of restricted practical use because it must be assumed either that the output is maximum or that the effort is sustained and uniform. Feelings of strain and effort, the introspective indicators of mental work, are useful in some cases, but inconsistent and mutually contradictory in others. They are not the invariable conditions of mental work, but only its occasional by-products.

There remains the possibility of defining mental work in terms of organic metabolism as determined by calorimetric observations or by chemical analysis of the products of organic combustion. Theoretically, all nervous activity involves metabolism; and the question is whether we are able to correlate mental activity with metabolism and, consequently, to express it in thermodynamic units. The respiration calorimeter is not suited for psychological experimentation; but we may use the pulse-rate as a measure of metabolism because calorimetric experiments have shown that it is, under certain restrictions as to blood-pressure, proportional to metabolism, not only during predominantly physical work but also during long sustained mental activity. This correspondence is much closer for long than for short periods. This limits the scope of such investigations which, moreover, remain preliminary so long as they are not supplemented by calorimetric determinations. If we could experimentally eliminate all extraneous activities we might use the pulse-rate for arranging mental as well as physical activities along a general metabolic scale; and it is conceivable that these co-efficients of metabolism, expressed in thermodynamic units, may become a means of analysing the obscure conditions of mental life which introspection can not reach.

In the traditional experiments on the influence of mental phenomena on circulation, little attention is given to the initial state of the subject which is necessarily influenced by the instrumental arrangement and by the unfamiliar surroundings. The following specifications are

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<sup>1</sup>RAYMOND DODGE. *Mental Work: A Study in Psycho-dynamics.* *Psychol. Rev.* XX, 1913, 1-42.

formulated for the recording instrument to be employed: 1. Legibility of every pulse-wave to at least 0.005 seconds; 2. evidence, in the curves, of changes in blood-pressure; 3. simple and inconspicuous attachment to the subject; 4. transmission to an adjoining room so that the recording devices may not become an object of the subject's attention; 5. greatest possible freedom of movement for the subject; 6. frictionless recorder; 7. comparable records in two different sittings of the same individual as well as of different individuals. The experimental problem thus defined was solved by a telephone-galvanometer-sphygmograph consisting of a telephone receiver, the circuit of which was completed by the string of a galvanometer. The movements of the steel armature, which rested on an insulating surface directly against the skin over the artery, were sufficient to induce measurable currents in the coils of the magnets. The receiver was placed over the temporal artery, which affords the best conditions for a permanent attachment; it was kept in place by an elastic band. The movements of the needle were photographed by means of Dodge's falling-plate device. The instrument gives almost complete freedom of movement to the subject; and only very violent bodily activity disturbs the record. The most serious limitation of the instrument is that the height of the pulse-waves in different records can not be directly compared because it depends on the initial distance of the armature from the magnets and on the velocity of its motion.

Physical activity is correlated with an accelerated pulse, from which it follows that physical and mental work may be equated. Measurements of their respective metabolism show that the mental processes of multiplication, reading, or learning a series of twelve familiar words involve less work than 6, 4, or 2 genuflections; they are closely approximated by the raising of a one-pound weight once every four seconds. The relatively small energy transformations of mental work are not surprising since electrical and heat products of nervous tissue in action are relatively small in comparison to analogous products of muscular action. We call attention to Dodge's treatment of the pulse-waves, which is novel. He gives what practically amounts to tables of distribution of the wave-length for the different experiments and attempts to draw conclusions from these data. He then tries to explain his results in the light of information as to the way in which the subjects behaved before and during the experiment.

The paper sets a new standard for sphygmographic experiments. It shows that the subject may be given complete freedom of motion, which is absolutely essential for experiments which last some time. Keeping the subject motionless in the same position produces a strain and interferes with the normal development of the processes under observation, not to mention that in a somewhat prolonged sitting the subject may fall asleep, thereby depriving the experiment of all psychological interest. It seems that this requirement disposes of the plethysmograph and definitely settles the question in favor of the sphygmograph as the instrument to be used in future investigations along this line.

It may be questioned whether Dodge's instrument furnishes the final solution of the problem. Some of his curves differ so widely from those traced by means of the Marey sphygmograph that one can not let them pass unchallenged. The experimental critique by Mach and Marey has shown that the sphygmographic curves are essentially correct; and any new instrument which traces them differently must remain under suspicion until evidence is forthcoming that the tracings

are correct. Past failures make Dodge's success appear very remarkable; and the most surprising thing, as a matter of fact, is that his curves are as good as they are. I am indebted to the head of one of the largest firms of instrument makers in Germany for the information that within the last two years he has refused to accept ten offers to construct electric recording devices of the telephone type because past experience led him to believe that such an instrument would not work. The mechanical reason is that vibrations of small amplitude, though they may be of even very high frequency, are transmitted correctly by the telephone; but the telephonic transmission of motions of considerable amplitude is inaccurate. It may be that Dodge just struck the happy medium in certain cases, but failed to do so in others.

Dodge's attempt to make a statistical study of the pulse-waves is equally significant; and it is my opinion that the solution of the problem can be reached only in this way. One of the most commonplace observations in plethysmographic and sphygmographic experiments is that certain variations of the curve occur in a great many cases, but fail to occur in other cases, although the objective and the subjective conditions seem to be exactly alike. It is bootless to say that the conditions must have been different, because if they had been alike they would have produced the same result. Indeed, if we were in possession of all of the information necessary to distinguish between these groups of conditions, the difficulty would not exist at all, and all further argument would be futile. The problem is to find out the differences between these groups of conditions which seem to be exactly alike in so far as our knowledge goes and which, nevertheless, produce different results. It seems a rather obvious plan to apply those notions which have proved useful in the study of similar problems in psychophysics, and to eliminate these accidental variations by a statistical treatment of the results.

Such an investigation will, in all probability, require a much closer analysis of the pulse-curves than has hitherto been attempted; and it will be necessary to study not only the length and the height of the curves but also their shape. Wave-form depends upon a number of conditions which we can unravel to some extent at least. The elasticity of the arterial wall, the action of the heart, and the blood flow through the arterial system all have a definite influence on the form of the sphygmographic curve and they may be determined from it. It will be necessary to study not only the single elevations but also the variations in form which appear in the course of an experiment. This requires a considerable number of measurements which could not be obtained without great difficulty from curves of the size which can be traced today; and it seems that such an investigation could not be undertaken until our technique has been so improved as to enable us to trace accurate curves of considerable size.

It is not possible to discuss fully Dodge's specifications for the ideal recording instrument, because this would open up the whole question of the method of expression. Specifications 3, 4, 5, and 7 will very likely meet with undisputed approval; number 2 might be omitted, since several physiologists believe that the pulse-curves do not show any evidence of blood-pressure. Number 1 seems to demand a degree of accuracy which really is not needed. Dodge gives his tables of distribution in intervals of 0.020 sec., which make it appear to be a waste of energy to trace curves which could be measured with an accuracy of 0.001 sec. Number 6 is only another way of insisting on

photographic registration, since this is the only recording device which is really frictionless. This advantage really amounts to very little so long as only the length of the waves is measured because an ordinary recording device would give this quantity with sufficient accuracy. It may be of some consequence if the form of the curve is taken into consideration; but even in this case one may rely on the observations of Mach and Marey that friction does not materially affect the tracing so long as it does not exceed certain limits in proportion to the impulse. Photographic registration, on the other hand, has the very serious drawback of being expensive and a little cumbersome, as is proved by the fact that Dodge took records during a very small part of the experiments only. I am inclined to substitute for number 6 the specification that the tracing must be correct with a sufficient degree of exactitude,—this to be tested by Mach's method; and for 1, that the curves be so large that they can be measured conveniently. I also favor the requirement that, for purposes of investigation, subjects highly trained in introspection should be used exclusively, for only in this case will one be able to correlate definite mental events with definite changes in the pulse-curves.

It may be doubted whether it is an improvement of the status of sphymographic experiments to link them up with the study of energy transformations. Any information as to energy transformations which take place as concomitants of mental processes certainly would be an extremely valuable addition to our knowledge; but no such information is at present available nor is it likely to be forthcoming soon, for calorimetric experimentation in psychology will be at least as difficult as plethysmographic or sphymographic analysis. One may venture to believe that the difficulties will be even greater because to the difficulties of isolating the mental processes will be added the difficulties of apportioning the proper amounts of energy to the different physiological processes going on at the same time. That this is not an imaginary but a very real difficulty is proved by the recent discussions between Lehmann, Exner and Hellpach on the notion of metabolism (as measured by the amount of carbonic acid secreted) during mental work. Dodge's hope that *thermodynamic analysis* may one day help us toward a better understanding of processes not accessible to introspection refers to an ideal state of knowledge, and has nothing to do with the present state of affairs. Similar hopes were expressed in regard to plethysmographic experiments; but the prophets have been singularly reticent of late.

Let us suppose, however, that all the experimental difficulties have been overcome successfully, and that we know the energy transformations corresponding to every mental process. Does that really give us a measure of mental work? The principle of the conservation of energy compels us to refer these energy transformations to the concomitant physiological processes, in which energy can be neither gained nor lost. The entire amount of energy, as determined by calorimetric measurement, is consumed by them; and no energy remains to be referred to the corresponding psychical processes. We have a complete understanding of the energy transformations which are involved in the physiological processes; but we are as far from a dynamic psychology as ever, for we can not equate mental work with physical energy, although we can correlate them. From this it follows that psychodynamics as defined by Dodge has the same limitations as the doctrine of some thirty years ago that psychology must express mental events in terms of brain-physiology.

The following interesting glimpse of individual psychology may be mentioned. On page 2 Dodge states that "actual science knows no other extension of knowledge except correlations." This is, of course, the well-known Mach-Pearson view which denies causal relations, and insists that the study of relations is the real field of science. In his paper on the "Theory and Limitations of Introspection"<sup>1</sup> Dodge finds fault with introspection because it "has never been able to fill out the causal relations of any fact of consciousness." The contradiction between these two statements is obvious; and it is all the more surprising because only a few months elapsed between the appearance of the two articles. It seems that the author vacillated between these two views as often happens when our ways of thinking are not yet fully adapted to a new idea. This is especially true in a case where the new view makes such large demands on the adaptability of the subject as Pearson and Mach do when they ask us to give up the notion of causal connection and substitute for it the idea of functional dependence. A sentence on page 7 of the article under discussion might lead one to believe that transfers of energy which condition consciousness are the causal relations which Dodge had in mind when he denied that introspection could fill out the causal relations of the facts of consciousness.

Several passages of the paper are sure to arouse the antagonism of the reader who expects to find a calm argumentation. Expressions like "Our entire experimental knowledge of mental fatigue is on the yardstick basis" or "dynamic psychology has long been cast into the rôle of the ill-favored sister" smack of the sensational style of William James, which is not entirely pleasant to everybody. A patriotic plea for psychodynamics as a truly American science makes a curious impression at the beginning of a scientific article. The translation of the German *Bahnung* by facilitation does not seem correct; the term reinforcement appears to be in more general use. It is true that the questions connected with the problem of mental work have not aroused the interest they deserve; but it is misleading to mention Lehman as the only neglected worker in the field. It would have been not more than just to mention the name of Charles Henry, who has worked in this field for almost twenty years and who lately published a very remarkable book on *Sensation et énergie*. Dodge certainly fails to do justice to the views of the extreme introspectionists; and it would not be surprising if his views call forth a pointed answer from that quarter where every item of information which is not of purely introspective nature is ruled out of psychology. It seems to me that the only standpoint which equally takes both aspects of the problem into account is the one lately developed by psychophysics; its field is here defined as the study of those mental phenomena which are directly accessible to introspection, and of the processes and objects connected with them. This definition at once specifies the immediate object of psychology and provides a legitimate place for the information about correlates of mental phenomena which we can obtain from physiology and comparative psychology.

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<sup>1</sup> This JOURNAL, XXIII., 1912. 226.